# Chapter 9
# The monks of Ely and the records of the survey – Part II

In dealing with xEl we have two dangers to avoid. On one side, we risk being distracted by questions which, however important they may be for the history of Ely, or for the history of Cambridgeshire, are not especially relevant to the survey. On the other side, we risk being trapped by questions which can be discussed interminably, without our making any progress towards an answer. If we want to stay clear of these dangers, we shall have to choose a course carefully – and having made our choice we shall have to travel fast.

I start by discussing two technical matters. First, I consider the anatomy of the text, as it appears in the earliest surviving copy. Second, I consider the relationships between this and the other two copies. After that, I propose to discuss some aspects of the evidence, selected because they are significant, and because it is possible to draw conclusions about them with a fair degree of confidence.

# 1

The contents of the text, as it appears in T, divide into 17 segments (Table 28). Additional segments appear in U and V. It is not impossible that some of these are authentic parts of xEl accidentally missing from T; but there is (unless someone can prove otherwise) no justification for regarding them as such. I attach no significance to them, except as indications that xEl was thought important – important enough for additions like these to become attracted to it. Some portions of the text as it appears in T are fairly sure to be accretions of the same kind, dating from between the 1080s and the 1130s.[1] The most obviously incongruous segment is the list of parish churches in Norfolk (122r), which has no ostensible connection with the survey: presumably there was some space here in the original, and somebody decided that the space might as well be made use of for inserting a copy of this list. Six other segments, it seems to me, are not, or cannot safely be assumed to be, parts of the original text, and I discuss these briefly, one by one, before dealing with the core components.

(i) The title. In all three copies, xEl begins with a long ex-planatory title (92r) – a famous passage, frequently quoted, not infrequently out of context.[2] There are two odd things about this title, considered as a title for xEl: it seems to be speaking of a single county, whereas xEl speaks of six, and it seems to be speaking of an entire county, whereas xEl speaks only of those manors in which Ely had an interest. This title does not mention Ely, or Saint Audrey, or the abbot and monks.[3] It would in fact be much more suitable as a title for B-Ca; but we have no evidence for making that link, or at least not for making it directly. At all events, we have reason to doubt whether this title is properly connected with xEl.

(ii) The lists of jurors. In T and V, the title is followed by a stretch of text reciting the names of the jurors (eight for each ordinary hundred, twice as many for a double hundred) for fourteen Cambridgeshire hundreds and three Hertfordshire hundreds (92r–4v). In U this stretch was originally omitted; then it was supplied, by a different scribe, at the end of the preceding quire. What we take this to mean will depend on how we construe the relationship between T and U (see below), but there must have been some room for doubt, in the mind of at least one Ely monk, whether this segment ought to be included or not. Nevertheless, it must certainly derive, as xEl-Ca does, from B-Ca, that being the only version of the survey text in which the jurors' names were to be found. At some stage (not before the 1080s, not after the 1130s), the Ely monks must have got their hands on a portion of the B text which covered two of the counties of interest to them, but (so it seems) not the other four. Scanning through this, somebody extracted the lists of jurors' names, stringing them together to make this stretch of text.[4]

(iii) A statistical digest covering the manors held by the abbey in domain, reporting five numbers for each: domain

[1] Three segments, Ca, Ht and Nk-1, end with a note reporting the total value: *De toto quod habemus in tota scira . . .* (110v, 114r, 122r). (For Ca and Ht the total is the same as in segment xEl-s; for Nk it is a slightly smaller amount.) The first person plural – 'we' meaning the monks – proves these notes to be additions made in Ely; but this is the only context where it occurs.

[2] The passage begins: *HIC SUBSCRIBITUR INQUISITIO TERRARUM, quomodo barones regis inquirunt . . .* (T-92r). At the very least, one should insist on having this passage quoted from T, not from V.

[3] By contrast, the table of contents inserted into U (above, p. 87) refers to xEl as a *Descriptio terrarum ęcclesię sancte æd'eld' in uolumen protensum.* (I do not understand what was meant by the last three words, 'into a stretched-out volume'.)

[4] From his treatment of Hertfordshire, it appears that this scribe's intention was to copy the lists only for those hundreds which were of interest to Ely. In Cambridgeshire, if that was his plan, he had one hundred to omit – and he did indeed omit one hundred, but not the right one. Instead of omitting Whittlesford, he omitted the hundred before it. By accident or on purpose, he also changed the order from that existing in B-Ca, making two transpositions.

| xEl / T | segment | Hamilton 1876 |
|---|---|---|
| 92r | title | 97 |
| 92r–4v | jurors (CaHt) | 97–101 |
| 94v–110v | xEl-Ca | 101–21 |
| 110v–13r | xEl-s (CaHtExNkSkHu) | 121–4 |
| 113r–14r | xEl-Ht | 124–5 |
| 114r–15r | xEl-Ex-1 | 125–7 |
| 115r–17r | xEl-Ex-2 | 127–30 |
| 117r–22r | xEl-Nk-1 | 130–6 |
| 122r | parish churches (Nk) | 136–7 |
| 122r–5r | xEl-Nk-2 | 137–41 |
| 125r–33r | xEl-Sk-2 | 141–53 |
| 133r–42r | xEl-Sk-1 | 153–66 |
| 142r–3r | xEl-Hu | 166–7 |
| 143v–5v | domain manors | 168–73 |
| 146r–v | villains' ploughs | 174–5 |
| 146v–9r | tenancies | 175–82 |
| 149r–v | Hamo de Sancto Claro | 182–3 |

Table 28. Contents of xEl as represented in T.

ploughs, men's ploughs, villains, bordars, slaves (143v–5v). The factual information is nearly all derived from xEl, but – as the title explains (this title does mention Saint Audrey) – the entries have been rearranged to bring together all the manors administered by the same reeve (which means that they have been rearranged very drastically).[5] Subtotals are entered at the end of each reeveship, if it comprised more than one manor, and that supplies a check on the numerical data.

This and the next three segments – not thought worth printing by Ellis (1816) – are all derived from xEl, but the presumption is that they are independent from T and the other copies of the complete text; so they may have some textual value, each within its own narrow compass.[6] It also has to be remembered, however, that they may have been edited, more or less extensively, by scribes drawing from other sources available in Ely.[7]

(iv) Another statistical digest, reporting for each manor a single figure, the number of villains' ploughs (146r–v).[8]

(v) A schedule of the abbey's non-domain lands, categorized as 'thegnland' and 'soke', organized tenant by tenant and county by county (146v–9r).

(vi) A schedule of the Ely lands held by Hamo de Sancto Claro (149r–v). In T there is a change of format at the start of this section, and perhaps a change of appearance in the script. Apart from the heading, it consists entirely of excerpts from xEl-Sk-2 relating to the lands which in 1086 were held by the bishop of Bayeux. But the heading proves that it was not compiled till much later, in or after the 1120s.[9]

The end of this schedule is the end of the text, as it appears in T; four blank pages follow. In each of the other two copies, U and V, there is more to follow. In U two fairly long documents occur at this point.[10] Scribe U1, seemingly without hesitation, continues with a document listing the barons who are still in possession of some of Ely's lands – lands which they ought to hold from the church or else not hold at all (207v–9v, ed. Hamilton 1876, pp. 184–9). Scribe U3, leaving one page blank, adds a document listing the lands to which the monks (we are told) succeeded in proving their claim, at a trial held by bishop Goisfrid and others, but of which they have still not been able to get possession (210v–13v, ed. Hamilton 1876, pp. 192–5, ed. Bates 1998, pp. 413–17).[11] Two shorter documents occur in V: a list of the monks' fisheries (69ra–b, ed. Hamilton 1876, pp. 190–1), loosely related to a list that was added in T (156v), and a memorandum explaining that a cartload of lead from the Peak amounts to only four-fifths of a London cartload (69rb, p. 191) – a fact worth knowing, no doubt, if one had a church to roof, but not relevant for us.

## 2

Up to a point, it is easy to work out the relationships between these three surviving copies of xEl. T is the earliest copy, and – at least in a superficial sense – very obviously the best one. By failing to base their editions on T, Ellis and Hamilton went wrong before they started; the next editor of xEl, one hopes, will not make the same mistake. For the other two copies, the basic pattern is this: U very frequently differs from T and V; V very frequently differs from T and U; but it never happens that U and V agree in differing significantly from T. In this passage, for example:

T: *T'c & post: ual' xx lib', 7 modo xxx lib'* (115r)

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[5] The title is perfectly explicit about this: *et hęc distinguuntur sicut prepositi tenent quisque preposituram suam* (T-143v). I do not know why Finn (1960, p. 397) made a mystery of it.

[6] There is a good example of this in the paragraph relating to Streetly, where xEl reads *ii c' & dm' h' in d'nio* (96r). From B / V, confirmed by DB (para. 5/15), it is clear that this ought to read *ii c' & dm' & dm' h' in d'nio*, 'two ploughs and a half and a half hide in domain'. The digest of xEl has the right number of ploughs, *ii c' et dm'* (143v). This tends to prove that the omission of *& dm'* was an error originating in T, and that the other two copies, because they share the error, both derive from T.

[7] Presumably that is where we look for an explanation of the disconcerting fact that this digest reports the presence of slaves on the manors in Huntingdonshire. Neither DB nor xEl mentions the existence of slaves; but somebody knew that they were there, and how many of them there were.

[8] In T it carries the title *UILLANORUM*. The scribe of V, not seeing the sense of this, changed it to *Nomina uillarum*.

[9] Hamo de Sancto Claro was given custody of the lands which reverted to the king after the death of Eudo Dapifer in 1119–20 (Farrer 1925, p. 168). Presumably this schedule was drawn up, then or soon afterwards, because there was some hope of persuading the king to give back the lands which had been stolen from Saint Audrey.

[10] They were discussed – how helpfully the reader must decide – by Finn (1960, pp. 398–407).

[11] The trial took place before 1075; this text, which does not pretend to be a contemporary record, is earlier than 1086.

U: *t'c ual' xx lib' 7 modo xxx^ta* (190r)
V: *Tunc & post: ual' uiginti li* (48vb)

there are two omissions in U, one different omission in V. For reasons already explained (above, p. 88), we can take it as certain that V was copied from T: it shares T's errors (as far as these can be detected), and has numerous errors of its own.[12] V's readings are of no textual value, and of interest only as a means of assessing this scribe's reliability. The sole question remaining to be answered, then, is the relationship between T and U.

This is one of the places where we risk being sucked into some interminable discussion. Round claimed to have proved that U is independent from T, and subsequent commentators have assumed this to be true; but the proof dissolves on inspection. Despite the emphatic language – 'A careful analyis … has satisfied me beyond question …' (Round 1895, p. 124) – it has to be remembered that Round was relying on Hamilton's apparatus, which is far from perfectly accurate. Only one passage was cited specifically by Round (1895, p. 131) as proof that U could not have been copied from T, because T has 'blunders' from which U is immune. Here is that passage, as it appears in the manuscripts:

T: *IN lolesuuorda: fuit quidam sochem', sub abb'e eli i h' & dm': tenuit t r ead', potuit dare pot' licent' eius sine soch'm'; & modo pi uicecomes, tenet eam sub abb'e eli. Valet x s* (103v)

U: *IN losewrd'a fuit quidam soch' sub abb'e ely i h' 7 dim', ten' t r e, pot' dare sine lic' eius sine soch', 7 modo pic' uicec' ten' eam sub abb'e ely, ual' x s* (184r)

There are indeed two errors in T, but it is silly to call them 'blunders'. They are mere slips – small mistakes, obvious to any reader, and easily put right by any copyist who has his wits about him (if he thinks himself at liberty to alter the text).[13] The readings found in U do indeed make better sense, but that does not prove anything at once. They could be original readings, preserved by U but garbled by T, as Round supposed them to be; alternatively they could be corrections made by an editorially minded scribe, copying, but not copying thoughtlessly, from T. This is not just to say that the evidence is ambiguous; on the contrary, as far as the first of T's 'blunders' is concerned, it is fairly certain that Round's interpretation is the wrong one. At this point, without much doubt, the original reading would have been *preter licentiam eius*, 'without his (the abbot's) permission'

– a phrase which occurs quite frequently – with *preter* written in such a way (*p't*) that a copyist might mistake it for *pot'* (to be read, one assumes, as *potest*).[14] That makes nonsense; and the U scribe, refusing to copy nonsense, substituted the word *sine*, which carries the right meaning, but is actually not the right word.

It is not difficult to find evidence which suggests, seems almost to prove, that U was not copied from T. In my experience, however, this evidence always turns out to be inconclusive, when it is looked at more closely. I cite just one example. In the paragraph relating to Wisbech, as it appears in T, the value clause reads: *Inter totum ual' c s, quand' rec': c s, t r e: lib'* (108v). The numeral which ought to come before *lib'* is missing.[15] In T this omission coincides with the end of a line; and that suggests that the error originated here, through a momentary lapse of concentration on the part of scribe T1. In that case we would expect to find the same error in any copy derived from T, but not in any other copy. If we look at V, what we find is a gap: *t r eaduuardi · · · lib'* (45ra).[16] Looking at U, at first sight we seem to find proof that this copy does not derive from T: *t r e vi lib'* (186v). But there is something odd about the spacing here. On closer inspection it appears that scribe U1 (like scribe V1) initially left a gap here, and then (unlike V1) inserted the numeral afterwards, not using up all the space that he had left for it. Instead of contradicting it, therefore, this evidence tends rather to confirm the view that U was copied from T, with the qualification that some of the errors inherited from T were then successfully corrected.[17]

The crucial point, I think, is this. Unlike scribe V1, the scribes who worked on manuscript U were not just making copies. They were participating – they were conscious of participating – in the creation of a work of literature. Of course they would not copy nonsense: if the source text needed to be knocked into shape, they were willing to treat it roughly. The question which we have been discussing, the relationship between xEl / T and xEl / U, should thus be subsumed into a larger question, the relationship between T and HEI / U; and that is an Ely question, not one for us. By reframing the question, I release myself from the obligation to answer it; but it seems to me that the author of HEI was working directly from T – from booklet 1 for the *Libellus*, from booklet 2 for the documents that he quotes,

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[12] One variant cited by Hamilton (1876, p. 137, note 23) would, if it were correctly reported, tend to disprove this conclusion. But in fact the words *modo xl sol'* are omitted only by U, not by both T and U. The V scribe, however, did occasionally allow himself to correct an obvious error. For instance, he corrected T's *Inter ual' x lib'* (109r) to *Inter totum ualet x li* (45rb), by analogy with other entries.

[13] Round also mentions a 'blunder' in V. This is the omission of *tenuit*, again the sort of error which could easily be corrected by a copyist of moderate intelligence.

[14] The error would have originated either in T's exemplar or in T itself. No manuscript has the reading quoted by Round, 'sine licentiam [sic]'. Scribe U1 wrote *sine lic'*; to all appearances he knew just as well as Round that *sine* takes the ablative.

[15] By consulting DB, we can find out what this number should be: *TRE: vi lib'* (DB-Ca-192ra).

[16] The gap means: 'Do not blame me for this. The error is my exemplar's fault, not mine. Feel free to supply the numeral if you can.'

[17] If scribe U1 had to go looking for the missing numeral, where might he have found it? There are two or three possible answers. He might have found it in T's exemplar, if that manuscript still existed. He might have found it in the manuscript which would later serve as the exemplar for V's booklet 2. Or he might have found it – just as we did – in DB-Ca's chapter 5, if Ely had procured a copy of that.

from booklet 3 for xEl. As far as xEl is concerned, I find nothing which could not be accounted for on the theory that U is a fair copy of an edited copy of T. In fact, the editing continued in U itself, where there are numerous blanks or erasures and marginal additions, and this manuscript is going to pose some serious problems for xEl's next editor.[18]

# 3

For the time being, it seems safe to proceed on the assumption that T is the only complete copy of xEl which has any textual value.[19] The excerpts listed above were presumably derived from T's exemplar (which, in the absence of evidence to the contrary, is presumably also the original), not from T itself; so these can be used, for what they are worth, as independent evidence. More remotely, T can be compared with the official versions of the survey text: against B for most of Cambridgeshire; against D for Essex, Norfolk and Suffolk; against DB for the whole of Cambridgeshire, and for Hertfordshire and Huntingdonshire. That gives us a lot of territory to cover, and I shall be driving through it as fast as I think safe.

To map out the route ahead, I list the core segments of xEl again, in a slightly different order (Table 29). Where there are two segments relating to the same county, one segment covers the manors of which Ely is safely in possession, and the other segment covers the manors of which Ely has lost – but still hopes to regain – possession. It seems clear that the pair of segments for Suffolk ought to be in the order shown here, like the matching pairs of segments for Essex and Norfolk, though in T the order is reversed. As for segment s, there is no reason for it to come between Ca and Ht, as it does in T: the logical place for it would be at the end (or else at the beginning). Wherever we put it, it differs in character from the other segments; it is also, in my judgment, the most valuable segment, and I keep it for last for that reason (below, pp. 103–6). The fact that the Sk segments have become transposed in T is a hint that xEl may originally have consisted of a batch of booklets, the order of which was liable to be perturbed. Nevertheless, the order of the counties, as they appear in T, is not haphazard: in segment s, the paragraphs which summarize the abbot's holdings run through the counties in precisely this order.

Because Ca is the only county where we can compare xEl with B, we are bound to start with this segment; but a word of caution is needed before we can begin. By analogy with Ex, Nk and Sk (in Ht and Hu the question did not arise), we would expect to find two segments for Ca as well. In fact

| xEl / T | segment | Hamilton 1876 |
|---|---|---|
| 94v–110v | xEl-Ca | 101–21 |
| 113r–14r | xEl-Ht | 124–5 |
| 114r–15r | xEl-Ex-1 | 125–7 |
| 115r–17r | xEl-Ex-2 | 127–30 |
| 117r–22r | xEl-Nk-1 | 130–6 |
| 122r–5r | xEl-Nk-2 | 137–41 |
| 133r–42r | xEl-Sk-1 | 153–66 |
| 125r–33r | xEl-Sk-2 | 141–53 |
| 142r–3r | xEl-Hu | 166–7 |
| 110v–13r | xEl-s (CaHtExNkSkHu) | 121–4 |

Table 29. Core segments of xEl as represented in T.

there is only one segment, and all the manors in which Ely had any interest are listed here, in strictly cadastral order, regardless of their current status. In some sense, therefore, perhaps in a significant sense, xEl-Ca is exceptional.

Many paragraphs of xEl-Ca are reproduced, in virtually the same order,[20] word for word or nearly so, from B-Ca. One example will be proof enough. I quote B's description of Willingham (para. 5/39), as it appears in V, and xEl's description, as it appears in T:

B / V-97vb: *IN hoc hundr' Wiuelingeham pro vii h' et dimid' se de t r e et modo: pro v.     Et de his vii h' et dim' tenet abb' de ely vii h', vii carrucis e' ibi t', iiᵉ c' et iiii h' in dominio, v carr' uillanis, xii uillani, viii cotarii, i seruus. Pratum vii carr'. Pastura ad pecuniam uille. De maris vi sol', iiii xx ou' ii xx por iᵘˢ runc'. In totis ualentiis ual' c sol' et qn' recep' totid' t r e viii li. Hoc manerium iacet et iacuit semper in eccl'ia S' Ædel de ely in dominio.     Et de his vii h' et dimid' tenet i sochemannus de comite alano i uirgam . . .     Et de his vii h' et dimid' Rogerus tenet de picoto uicecomite i uirgam . . .*

xEl / T-103r: *Wiuelincgaham pro vii h' & dm': se defendit tpr' R ead: & modo pro v h';* **In hac uilla** *ten₇ abb' eli: vii h'; vii c' ibi e' t'ra; ii c' & iiii h' in dominio; v c' hom'; xii (uill'i); viii (cot'); i (s); pratum ad vii c'; pastura ad pecc' uille; de marisca: vi s, quat' xx o; xxii p & i runc'. In totis ualentiis ual₇ c s, quand' rec' c s, tpr' R ead viii lib'; Hoc man' iacet & iacuit in ęccl'a S' Æld' in d'nio.*

Not only does xEl agree verbatim with B: it even begins by giving the total assessment for this village, an item of information which was only to be found in the B text. But then it simplifies things, replacing 'And of these seven hides and a half' with 'In this village', and focuses on the abbot's seven hides, ignoring the other two virgates. What could be clearer? The paragraph in xEl is an edited excerpt from B-Ca, put into this form by someone preoccupied by matters affecting Ely.[21]

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[18] As I understand it, what U represents is an attempt to construct an improved version of the xEl text, suitable for inclusion as an appendix to HEI. The attempt did not quite succeed. Scribe U3, we may think, when he recopied the end of HEI / U, ought also to have recopied xEl, but he did not bother to do so. The scribe who recopied HEI in its definitive form, HEI / W, did not include xEl at all.

[19] Any statement about xEl which begins 'In all three manuscripts . . .' is immediately to be distrusted – and so is the author who made it.

[20] The only difference in the order is that one whole hundred has moved: Radfield comes after Chilford, not, as in B, after Staine.

[21] Round (1895, p. 9) quoted a similar pair of paragraphs, the pair relating

Though the order of the paragraphs is consistently the same, the degree of verbal resemblance varies greatly. We have only to look at the previous paragraph (para. 26/48) to find a much looser relationship between xEl and B:

B / V-97rb-va: *IN hoc hundr' Oura pro xv h' se de t r e … Et de his xv h' tenet Rad' de hardeuuino ii h' et i uirgam … Hanc t'ram tenuerunt x sochemanni t r e. Et i*[us] *istorum: homo Abb'is de Ely fuit. Dimidiam h' habuit, non potuit dare neque uen. Et ii istorum homines predicti abb'is iii uirgas habuerunt. Vendere potuerunt, soca remansit abb'i. Et vii alii homines abb'is de Rames' fuerunt, i hidam habuerunt, uendere uel dare potuerunt: sine soca.*

xEl / T-103r: *IN ouro fuit quidam soch'm'* **nomine Stanhardus** *qui dm' h' habuit sub abb'e eli, non potuit ire ab eo, nec separare ab ęccl'a;* **& valet xx s** *& modo h't harduinus. Et alii ii*[o] *soch'm': iii v' habuer' potuer' dare l' uendere sine soc' cui uoluer', & modo ten7 harduinus,* **7 valet xv s.**

There is information in xEl which is not to be found in B, and even to the extent that the substance is the same the wording is divergent. It is possible, of course, that an Ely monk, reading the original, might occasionally have added a note in the margin reporting some relevant fact which he happened to be aware of; conversely it is possible that some phrases might have been omitted accidentally by scribe V1, while he was making his copy of B-Ca. But neither explanation is adequate for what has happened here. We are not dealing with casual additions in xEl or casual omissions in V. The whole entry has been reworded. What in V is a regular part of the B text is replaced in xEl by a paraphrase – a new version of the entry which agrees with B only loosely, and only up to a point.[22]

We are, I think, obliged to conclude that xEl-Ca as we have it is a palimpsest. The underlying text consists of the paragraphs – like the one for Willingham – which, in their wording as well as in their order, are manifest excerpts from B. But many of the entries have been overwritten with new paragraphs – like the one for the abbey's sokemen in Over

to Melbourn (para. 5/34). After noting some of the differences in wording, he added a careless remark which has done no little harm: 'These prove that verbal accuracy was not aimed at by the transcribers' (1895, p. 10). They do not prove that. The statement is true for at least one transcriber; it need not be true for both. These variants do not tell against the view that V is what it seems to be, a straightforward copy of B, and that xEl is also what it seems to be, a concatenation of edited extracts from B. There is no symmetry here: one text is vastly more reliable than the other. Another inept remark of Round's – 'the Domesday scribes appear to have revelled in the use of synonym and paraphrase' (1895, p. 26) – has also been frequently quoted. Why is it misbegotten epigrams like these that historians tend to repeat?

[22] Round (1895, p. 18) has a good example – an entry in B / V-84va which is almost identical with a pair of entries in DB (191rb, 198rb) but does not make sense as it stands. This is replaced in xEl / T-40ra by a longer sentence which explains the facts in more detail (and which makes it possible to understand how the error in the B text came about). The compiler of xEl, Round thought, appeared to have 'corrected the original return from his own knowledge of the facts' (1895, p. 19); but where did this knowledge come from? Not out of the compiler's head, we may be sure.

– which incorporate additional information and are much more narrowly concentrated on Ely's own affairs. This second layer of text, I suppose, is the product of some further investigation, a partial, private survey commissioned by abbot Simeon; but this is an Ely question, and I do not propose to pursue it. From our point of view, this revision of the text has greatly reduced its value. With caution, we can use xEl to reconstruct the missing portions of B-Ca, and more vaguely to form an idea of the B text for other counties.[23] But for every paragraph the question has to be asked: is this something close to a word-for-word excerpt from the official survey text, or is it the product of some subsequent revision in Ely? To the extent that the B text survives, xEl is of no interest except for correcting errors introduced by V. For the rest, xEl is equivocal.

With the other five counties, we are on much weaker ground. In the absence of a straightforward copy of B, we have only two terms of comparison, xEl and either D or DB. The textual evidence, in these circumstances, is irredeemably ambiguous, just as it is for that portion of Ca where V is not available. It was Round's suggestion, with regard to Ex, Nk and Sk, that xEl was copied from the surviving D booklets for these counties: on this view, any facts reported in xEl which are not to be found in D should be explained away as interpolations made by Ely scribes. Whether we think this likely or not, I do not know that we have any sure way of disproving it. One piece of evidence, frequently cited as proof that xEl-Nk-1 could not have been copied from D-Nk, does not prove any such thing. It is instead a good illustration of the ambiguity intrinsic to the textual evidence, and I discuss it, briefly, for that reason.

As was first pointed out by Johnson (1906, p. 4), there is a paragraph in xEl-Nk-1 – the final paragraph, relating to the manor of Bergh (T-121v–2r) – which has no counterpart in D-Nk (apart from one portion of it, which in D forms a separate entry). Except that it comes at the end of its segment, this paragraph has no odd features: it seems to be an integral part of the text. At first sight, therefore, this seems to prove that xEl was derived from a version of the survey text earlier than D – a version from which this paragraph had not yet gone missing. The conclusion is probably right (see below), but this evidence does not prove it. If one looks at D, at the place where this entry would be expected to occur (D-Nk-214v19), one discovers two odd things. First, there is a hundred heading – *Heinesteda hund' dim'* – which would be right for Bergh, but is wrong for the paragraph (relating to Pulham in Earsham half-hundred) which actually follows this heading. Second, there is a caret mark at precisely this point – between the hundred heading and the place-name *Pullaham*. It seems clear that somebody noticed the omission; and if he was capable of detecting it, presumably he would also have been capable of correcting it. (The miss-

[23] That some revision has occurred in the other counties seems clear from examples like this: a sentence which in D reads *Rog' bigot tenet de abb'e, sed prius tenuit de rege* (D-Nk-214v) has expanded in xEl into *Hos ten' R. bigot de rege, set abb' diratiocinauit eos coram ep'o constantiensi, modo tenet eos predictus R. bigot de abbate* (xEl / T-121r).

```
a1  Armingford
c1  Chesterton
c2  Cheveley
c3  Chilford
e1  Ely
f1  Flendish
l1  Longstow
n1  Northstow
p1  Papworth
r1  Radfield
s1  Staine
s2  Staploe
t1  Thriplow
w1  Wetherley
w2  Whittlesford
```

Table 30. Cambridgeshire hundreds.

ing text – not just the omitted paragraph but also the omitted hundred heading – could have been added at any stage, as long as B-Nk or C-Nk was still available.) This hypothetical corrector did not write the missing passage in the margin; if he had done that, it would still be there. But he might perhaps have written it on an inserted slip; and the slip might have dropped out again, at some later date. If this evidence stood alone, it would be perfectly possible to argue, on Round's behalf, that xEl was copied from D during the span of time when this paragraph was to be found there – after the defect had been patched, before the patch fell away.[24]

# 4

Rather than wasting our time with evidence which is bound to be inconclusive, we need to find some alternative line of attack. For Cambridgeshire, because of the existence of V, it can be proved that xEl-Ca and DB-Ca both derive from B-Ca. Suppose that V did not exist. Would it still be possible to prove that xEl's source text for this county was the same cadastrally organized version of the survey text from which DB also derives? Can we find some argument which works successfully in Cambridgeshire – i.e. which produces the answer which we know to be the right one for this county – and then apply the same argument elsewhere? In this section I shall be going over the same ground that was covered by Sawyer (1955, pp. 186-90); but I have worked out all the evidence again for myself. (I should also like to think that I have explained things more clearly than Sawyer did.)

Because xEl-Ca consists of a single segment, with the manors on which Saint Audrey had some claim interspersed

among the manors of which she had possession, it does not need to be proved that the source text was organized cadastrally; that is obvious at once. The question is whether it was organized in the same way as B; and the simplest way of framing that question is to ask whether the hundreds were arranged in the same order.[25] By the time that they appear in D and DB, the hundreds have been chopped up into pieces, and the pieces have been distributed among the chapters where they belong. But in Cambridgeshire, as in many other counties, the original order is partially and approximately preserved within each separate chapter. It has long been understood, therefore, that by recombining the information available in different chapters of D or DB one can hope to reconstruct the order of the hundreds, as they appeared in B. This, by and large, is more easily said than done: the evidence is often inadequate and inconsistent.[26] Because of the complexity of the compilation process, perfect orderliness is hardly to be expected; at any stage, for any number of possible reasons, blocks of text may have got themselves rearranged. With luck, however, the message that we want to hear may be audible through the noise.

As it happens, Cambridgeshire is not one of the easiest counties to deal with. There are fifteen hundreds here (Table 30), and the information available from DB is not good enough for the sequence to be fully reconstructed.[27] The conclusions that I come to are these. (i) There are four hundreds (c2, r1, s1, s2) which seem to belong together but cannot be ordered satisfactorily with respect to one another. Usually they come at the front of the chapter, but in one case (chapter 14) they come at the back. (ii) With these four excluded, the remaining hundreds are reasonably well-behaved, and the order into which they fall is this:

```
DB-Ca:  f1 c3 w2 t1 a1 w1 l1 p1 n1 c1 e1
```

With the same four hundreds excluded, the order that exists in xEl is:

```
xEl-Ca:  f1 c3 t1 a1 w1 l1 p1 n1 c1 e1
```

Apart from the omission of the single hundred (w2) in which Ely had no interest, the two sequences are identical; and that is enough to prove that the source text for xEl-Ca

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[24] There is more to be said on the subject than this, but the rest is Ely business. The manor of Bergh, held by Godric Dapifer 'under the abbot', is only one half of the story: the other half concerns the manor of Apton. In xEl-Nk-2, we are not told who currently has possession of Apton; instead we are told that 'Saint Audrey is supposed to be getting this land in exchange for Bergh' (T-125r). In D-Nk, Apton is listed among Godric's manors, without any mention of Saint Audrey (203r–v). Ambiguity strikes again: which is the original and which is the edited version?

[25] In some other county it might happen that we could prove the weak conclusion (that the source text was organized cadastrally) without being able to prove the strong one (that it was organized in the same way as B). In fact this does not happen; so I shall be arguing directly for the strong conclusion.

[26] I do not have any algorithm for retrieving the latent order from a collection of partial sequences which are only roughly consistent with one another. An interactive program would be needed – one which identifies the most troublesome chapter, or the most troublesome hundred, asks for permission to delete it, and then tries again.

[27] I do not know whether anyone else has attempted to reconstruct the hundred order for Cambridgeshire from the evidence of DB alone. Sawyer did not try this: he took the order as given, on the evidence of B (patched up at the end from xEl), and then showed that this order is approximately preserved within chapters of DB (Sawyer 1955, p. 180). Hart (1974) did the same.

must have been B-Ca. (As we happen to know from V, the actual ordering in B was this:

```
s2 c2 s1 r1 f1 c3 w2 t1 a1 w1 l1 p1 n1 ....
```

and the partial sequence recovered from DB lines up exactly with that.)

We thus have a method, demonstrably successful in Cambridgeshire, which is capable of proving that xEl derives from B. But this method will only work under certain conditions. It will not work on a segment of xEl which corresponds approximately with a single chapter of D or DB. In such a segment, the order of the entries, and therefore the order of the hundreds, will be roughly the same regardless of whether the source text was cadastrally or feodally organized. Whether xEl's compiler had to work through B for himself, finding the paragraphs that he wanted, or whether he was able to profit from work already done by government scribes, there will be no difference in the outcome that we can recognize. Therefore we can only hope for the method to work on a segment of xEl which corresponds with multiple chapters of D or DB; and immediately that means that we are restricted to three segments, Ex-2, Nk-2 and Sk-2. These are (by chance) the same three counties for which D survives and DB does not. I discuss these segments in turn, in this order, which happens to be the most convenient one.

*Segment Ex-2.* The hundred order as it appeared in B-Ex is approximately reconstructable from D-Ex, and the sequence worked out by Round (1903, pp. 409–10) is mostly clear enough. There are some puzzling features, and several possible explanations for them; but these are Essex questions and need not detain us here. Only five hundreds are represented in Ex-2, and for these the order is reasonably certain, except that Uttlesford (u1) and Chelmsford (c2) should possibly be transposed. Either way, the result we get is negative (Fig. 9). There is no correlation between the order of the entries, as they appear in xEl, and the order of the hundreds, as they appeared in B. This is not to say that the order has been feodalized: that is not true either. Two entries relate to manors owned by Eudo Dapifer, but they have not been put together; two entries relate to manors owned by Goisfrid de Manneville, but they have not been put together either.

This segment of xEl is exceptional in two respects. In all the other segments, not excluding Ex-1, hundred headings are regularly present, barring some sporadic errors and omissions. In this segment the headings are consistently omitted, though in T the scribe did vaguely attempt to leave space for them. (The V scribe did the same thing, imitating his exemplar; possibly the T scribe too was imitating his exemplar.) More strikingly, this segment is unique in having its own title: *Has terras calumpniantur* [sic] *abbas de Eli secundum breues regis* (T-115r). It looks as if Ex-2 may have been compiled separately, at an earlier stage than the rest, and then subsumed into the larger project of which xEl
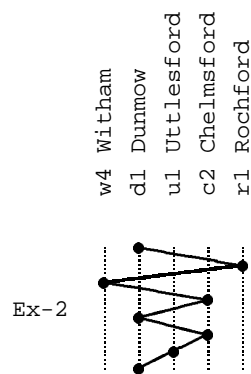


Figure 9. Order of the entries in xEl-Ex-2 mapped onto the order of hundreds in B-Ex.

is the result. The ordering of the entries, neither cadastral nor feodal, was presumably imposed by the man who made these excerpts: apparently he had some preconceived idea of the order into which they should be put.[28] However that may be, the method which we are using fails to prove that he was copying from B-Ex.

*Segment Nk-2.* There were 33 hundreds in Norfolk, and the order as it appeared in B-Nk can be reconstructed rather easily from D-Nk. Johnson (1906, p. 4) worked it out with almost complete success, making only one small misjudgment.[29] There were fourteen hundreds in which Ely had some interest, but only ten of these are represented by entries in xEl-Nk-2. The order of these entries correlates perfectly with the hundred order that existed in B-Nk (Fig. 10), except for two small discrepancies. One of the entries for Clackclose hundred (c1) has gone astray, tucking itself in at the end of the subsequent hundred; and the single entry for Henstead hundred (h2) has dropped back to the end of the hundred which ought to follow it. A few hiccups of this kind are only to be expected; they do not prevent us from seeing the overall picture. For Nk-2, it is clear, our method works very well: we can take it as a proven fact that this segment was copied from B-Nk.

The same correlation, somewhat disturbed towards the end,[30] exists in segment Nk-1. There it proves nothing; but I see no reason why we should not assume that what is demonstrably true for Nk-2 was true for Nk-1 as well. Both segments, I take it, were compiled by an Ely scribe who worked his way systematically through B-Nk, excerpting all the entries of interest to him and Saint Audrey. By the time that he was finished, he had made two booklets:

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[28] For example, perhaps he had been given a list of names of the places of interest to Ely and instructed to find the relevant entries in the survey text.

[29] Johnson's ordering transposes Greenhoe South (g3) with Grimshoe (g4). The evidence is inconsistent, but chapters 21 and 31 outweigh chapter 22. There is a misprint in the sequence reported by Sawyer (1955, p. 187): it ought to read either 1, 6, 1, 5, ... (if he was following Johnson) or 1, 5, 1, 6, ... (if he was making the same adjustment as me).

[30] The last entry, out of order, is the entry relating to Bergh (above, page 98).
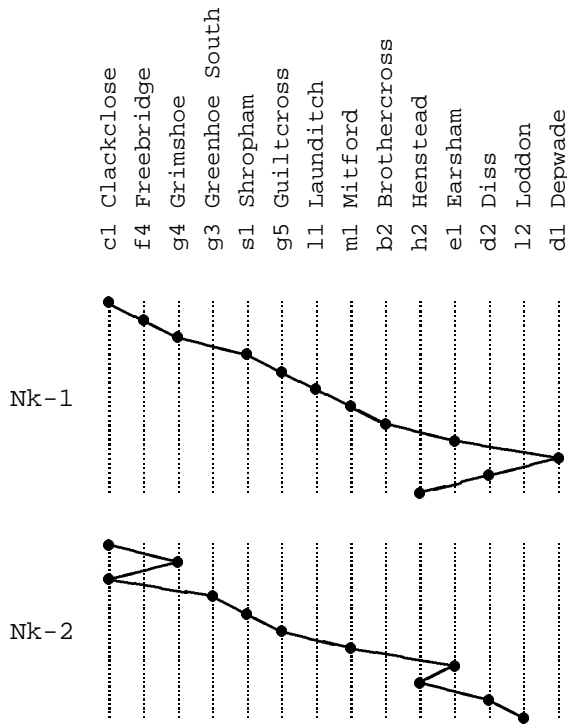
Figure 10. Order of the entries in xEl-Nk-1 and xEl-Nk-2 mapped onto the order of hundreds in B-Nk.

one for the abbot's manors, one for the manors belonging to other barons on which Ely had some claim. He might have scanned through B-Nk twice, making one booklet at a time; but the more sensible plan would be to scan through B-Nk just once, compiling the two booklets in parallel. It can be proved (see below) that this is how the booklets for Suffolk were compiled; so probably it happened here too. Each time the scribe came to a relevant entry, he read it through and made a decision. If Saint Audrey was securely in possession of this manor, he copied the entry into booklet 1; if Saint Audrey had a claim on the manor which she had not yet been able to make good, he copied the entry into booklet 2. And then he resumed his scan of B-Nk, looking for the next entry that he would need to copy. In the end, therefore, his booklet 1 was precisely analogous with one of the C booklets which had been or was about to be compiled by the treasury scribes, a booklet which would carry the title 'Land of the abbot of Ely in Norfolk' (corresponding with chapter 15 in D-Nk). But booklet 2 was made to the scribe's own formula.

*Segment Sk-2.* There were 25 hundreds in Suffolk, and the ordering as it stood in B-Sk can mostly be reconstructed without much effort.[31] Two hundreds, Blackbourn (b3) and Bradmere (b6), stand out as being very badly behaved, and I do not see how they can be fitted into the sequence;[32] a third hundred, Hartismere (h1), is also hard to handle,

seeming sometimes to come near the middle of the order, sometimes right at the end. Ely was interested in 19 of the Suffolk hundreds, and we can ignore the other six, which luckily include the two most troublesome cases. As for h1, the Ely chapter in D-Sk (chapter 21) puts it in the middle of the sequence, between p2 and r1, and no harm will be done if we follow that lead.[33]

Now, when we come to compare the order of the entries in segment Sk-2 with the hundred order reconstructed for B-Sk, we find a surprising pattern (Fig. 11). Piece by piece, we can recognize the same sort of correlation that we found in Nk-2, but here it is overwritten with some seemingly wild oscillations. A very similar pattern, with fewer irregularities, exists in segment Sk-1.[34] The patterns resemble one another, not just broadly, but also in some details – most notably in the fact that both segments have an entry for Ipswich hundred (i1) intercalated into Bosmere hundred (b5). In the source text, it seems, part of b5 was followed by i1, which was followed by the rest of b5. Reconstructing B from D, we cannot hope to recover small features like this – that is one of the reasons why we cannot expect to find perfect correlation – and xEl is telling us something here which otherwise we would not know. Whatever these oscillations mean, it seems certain already that the two xEl segments for Suffolk were compiled at the same time, by the same method, from the same source text.

Sawyer was at least on the verge of discovering these oscillations, but seems hardly to have been able to believe that they existed, let alone that they made any sense.[35] Finn (1960) seems also to have had some inkling of their existence.[36] The phenomenon is certainly real, and not beyond the reach of explanation. It results from a division of labour between two scribes, who take half of the B text each, and who jointly compile a pair of booklets like the pair for Norfolk. Scribe I takes the first portion of B, covering the hundreds from t1 to h1; scribe II takes the second portion, covering the hundreds from r1 to l2. Both start scanning through B; whenever one of them comes across an interesting paragraph, he copies it into the appropriate booklet; and the outcome is that, in both booklets, paragraphs copied by scribe I from his portion of B alternate with paragraphs copied by scribe II from his.[37] If the origi-

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[31] Sawyer (1955, p. 189) seems to have thought that he could reconstruct the order entirely, but does not say how he managed it.

[32] They are anomalous in other ways too (Davis 1954, pp. xxvi–ix).

[33] The reader who doubts that should redraw Figure 11, moving h1 to the end, and then wait to see what difference this makes to the following discussion. (The answer will turn out to be: no significant difference.)

[34] The data tabulated by Sawyer (1955, p. 189) can be mapped onto Fig. 11 by relabelling the hundreds *A, B, C, … , I, K, … , T.*

[35] 'It is tempting to see … two sequences intermingled, but this would be difficult to prove and hard to imagine in practice' (Sawyer 1955, p. 189). In fact, the result seen here is like the result that one gets by performing a riffle-shuffle on a pack of cards; but that is a simile, not an explanation.

[36] 'In Suffolk the [xEl] entries for each of five adjacent Hundreds [b5, c2, c1, w2, l2] are frequently not together, Hundred by Hundred' (Finn 1960, p. 389). The word 'adjacent' goes to show that he was seeking inspiration in the wrong place. Instead of looking at the text, he was looking at a map.

[37] This is a just-so story, and there are other ways of telling it. We might suppose instead that one man did all the scanning and the other man did
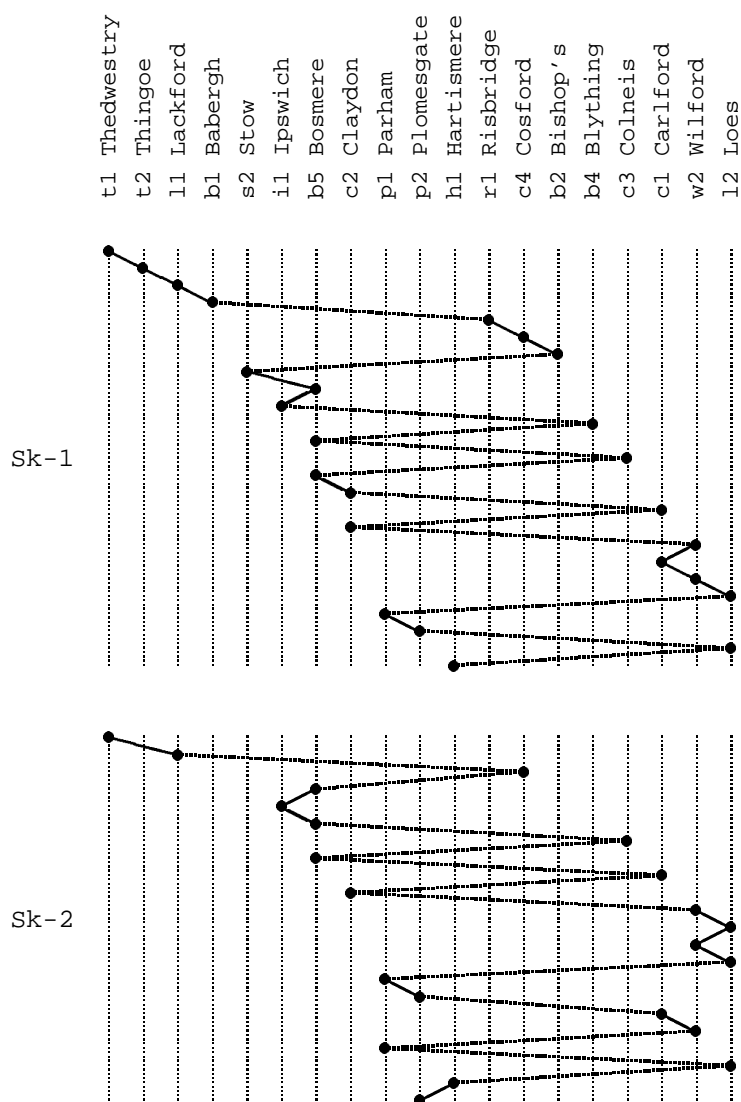
Figure 11. Order of the entries in xEl-Sk-1 and xEl-Sk-2 mapped onto the order of hundreds in B-Sk.

nal booklets survived, it would be obvious at once from the changes of hand that the oscillations are a by-product of this division of labour; even without the originals, thanks to the existence of D we can still work out what was happening.

To illustrate the point, I work things out more fully for booklet 1, i.e. the original for segment xEl-Sk-1 (Table 31). The paragraphs are identified with the corresponding paragraphs in D-Sk's chapter 21, as they were numbered by Rumble (1986).[38] Scribe I writes the first block of text

(paras. 1–11); scribe II writes the second block (paras. 40–6); then scribe I takes over again. If we read this table row by row, we may think that the sequence is chaotic; if we read it column by column, we discover that the order of the entries is approximately the same as in D-Sk, and that the order of the hundreds is approximately the same as the order reconstructed for B-Sk. The reader is welcome to draw up a similar table covering segment Sk-2.

Why the scribes chose to collaborate in this manner – why they chose to make one pair of booklets together, rather than each of them making a pair by himself – is another question. On the face of it, the plan is not a sensible one, because the scribes will get in one another's way. From time to time, one of them will come across a paragraph which he needs to copy, only to find that the booklet into which he needs to copy it is already being used by his colleague. This would

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all the copying. At any moment (except in the initial phase, when there is nothing yet for Writer to do), Finder is scanning through one portion of the B text, marking those entries that Writer will need to copy, and Writer is making his way through the other portion, copying those entries that Finder has already marked. Every so often, they change places (or exchange their portions of the B text, which comes to the same thing), and oscillation results. This is not a different conjecture; it is the same conjecture expressed in a different way.

[38] I have simplified things slightly by ignoring some small transpositions. In hundred c3, for example, the actual sequence is paras. 48, 50, 51, 49, 52, but I write this as paras. 48–52. It will be noticed that there are two entries here, near the bottom of the first column, which D-Sk puts into a different chapter.

| scribe I | hundreds | scribe II |
|---|---|---|
| 21/1–3 | t1 | |
| 4 | t2 | |
| 5–9 | l1 | |
| 10–11 | b1 | |
| | r1 | 21/40–1 |
| | c4 | 42–4 |
| | b2 | 45–6 |
| 12–14 | s2 | |
| 16–21 | b5 | |
| 15 | i1 | |
| | b4 | 47 |
| 22 | b5 | |
| | c3 | 48–52 |
| 23–5 | b5 | |
| 26–30 | c2 | |
| | c1 | 53–5 |
| 31–5 | c2 | |
| | w2 | 71–92 |
| | c1 | 56–70 |
| | w2 | 93–4 |
| | l2 | 95–100 |
| 21/36, 67/5–6 | p1 | |
| 21/38 | p2 | |
| | l2 | 101–4 |
| 39 | h1 | |

Table 31. Oscillations in segment xEl-Sk-1.

probably not be a serious problem,[39] but it could have been avoided altogether if each scribe had made his own pair of booklets. So we are left wondering why the less efficient plan was preferred. (The answer is, I would guess, that scribe I wanted to keep a close eye on the work that scribe II was doing.)

Whatever the reason for its adoption, the plan is a recipe for entropy: it shuffles hundreds together in an arbitrary way. But perhaps there was more to the plan than meets the eye. I suspect – but cannot hope to prove – that it may have been intended to make a fair copy later, and to put the paragraphs back into their proper order at that stage. Perhaps scribe I was intending to do that himself; but let us suppose (to make things harder for ourselves) that the task was going to be assigned to somebody else, scribe III. As long as scribe III understands what he has to do, as long as he can distinguish scribe I's writing from scribe II's (he should pay attention to the hundred headings too), he will have no difficulty in achieving the desired result. For each booklet in turn, he makes two passes through his exemplar: on the first pass he copies all paragraphs written by scribe I, on the second pass all paragraphs written by scribe II.

And that will do the trick.[40] In the event, it seems, the fair copy did not get made – or at least it did not get made till fifty years later, when scribe T1 did the job. By that time, not only were the booklets out of order: the fact that they needed special treatment had been forgotten, and scribe T1 just copied them as he found them.

For present purposes, these oscillations are important only because they make it certain that booklet 1 was compiled simultaneously with booklet 2. Without this evidence, though we might be willing to assume that, we would not be able to prove it. As far as booklet 2 is concerned, the oscillations are ultimately irrelevant. It is the piecemeal correlation between the order observable in xEl and the order reconstructable for B which proves the point which we were hoping to decide: that segment Sk-2 must have been derived from the B text – or, if we think it necessary to state the conclusion more diffidently, from a B-like manuscript which, because not demonstrably different from B, was presumably identical with it.[41]

Thus it can be proved, directly for segments Nk-2 and Sk-2, indirectly for segment Sk-1, that xEl is derived from B. The same is self-evidently true for segment Ca. Given these facts, it seems a fair conclusion – pending proof to the contrary – that the same applies to every segment of xEl. The basis for this conclusion is not as strong as it might be, because there is clearly some degree of heterogeneity among the segments, and accordingly some room for doubt as to whether what is true for one is true for all. That said, I cannot see any reason why we should be reluctant to think that the descriptive segments of xEl were all derived, as some of them certainly were, from the B version of the survey text. Conversely the evidence which we have developed from xEl can be taken to prove the existence of the B text, not only for Cambridgeshire, but also for Norfolk and Suffolk. Since the existence of the B text is also implied, by the surviving C booklets, for five counties on the other side of England, there is another obvious conclusion which it would be perverse not to draw: the B text existed originally for every county.

# 5

One segment remains to be discussed. I have not yet explained why segment s, despite its different character, should be accepted as part of the original xEl text, and why

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[39] In these circumstances, the scribe does not have to stop working. He can insert a bookmark at the place where he has found an entry and then continue scanning. When the booklet that he has been waiting for becomes available, he goes back and copies the entry that he found in the first place, plus any other entries that he has found and marked in the interim. And then he resumes his scan from the point at which he broke off.

[40] It is a nice question whether xEl's next editor would be justified in rearranging the Suffolk segments along the same lines as scribe III. If this were known for certain to have been the original intention, an editor would have no choice but to respect it.

[41] That is the conclusion for which Sawyer seems to be heading; but then at the very last moment he veers away. The source text, he decides, was not B itself: it was a copy of B in which the entries had already been reorganized to some extent. He had discovered, he thought, 'a stage of the enquiry' – of the compilation process, it might be better to say – 'which has not yet been noticed' (Sawyer 1955, p. 188). This conclusion is a non sequitur; but since it has been generally ignored, there seems to be no point in explaining why.

it is especially significant. That is what I now propose to do.

Segment s is itself segmented. It consists of sixteen paragraphs, the contents of which can be summarized as follows:

(1) The abbot of Ely in Cambridgeshire
(2) Picot, Hardwin, Wido and others in the same: Ely thegnlands
(3) The same in the same: Ely soke
(4) The abbot of Ely in Hertfordshire
(5) The same in Essex
(6) The same in Norfolk
(7) The same in Suffolk
(8) The same in Huntingdonshire
(9) Picot the sheriff in Cambridgeshire
(10) The same in the same: Ely thegnlands
(11) The same in the same: Ely soke
(12) Hardwin de Escalers in Cambridgeshire
(13) The same in Hertfordshire
(14) The same in Cambridgeshire: Ely thegnlands
(15) The same in the same: Ely soke
(16) Wido de Rainbuedcurt in Cambridgeshire: Ely thegnlands, Ely soke

Each paragraph begins by giving the name of the baron and the name of the county in question; then, for each of a series of designated categories of information (see below), it reports the total for this baron in this county. For some categories, but not for all, it distinguishes between the manors held by the baron in domain and the manors held by his knights. Trying to anticipate the sort of questions which might be put to him, the compiler worked out the answers in advance. Somebody, for example, might want to know the total assessment for all the manors held in domain by sheriff Picot in Cambridgeshire. Paragraph (9) supplies the answer (58.125 hides). Somebody might want to know the aggregate number of domain ploughs on all the manors held by Picot's knights in the same county. The same paragraph supplies the answer to this question too (17.5 ploughs). There are some slight variations (in the wording, in the order of the last two items), and in some cases the scheme is simplified because the distinction between domain and non-domain manors does not apply; but on the whole these paragraphs all conform closely to the same underlying pattern.

Some special treatment was required for the lands of a monastery like Ely, because of the complexity of the tenurial arrangements existing here. There are separate paragraphs for the thegnlands (para. 2) and the soke (para. 3); the totals reported here include the subtotals reported later for three individual tenants, Picot the sheriff (paras. 10–11), Hardwin de Escalers (paras. 14–15), and Wido de Rainbuedcurt (para. 16).[42] Because of the prominent place

that is given to Ely's affairs, it has often been casually assumed that segment s originated in Ely; but that, as Finn (1960, p. 390) pointed out, is manifestly not the case. The monks of Ely had no interest in knowing the aggregate statistics for the manors in Cambridgeshire that were held by the knights of Hardwin de Escalers (para. 12). They were interested in knowing, for instance, that one of Hardwin's men, Radulf by name, owned a manor of 2.25 hides in the village of Over, because this manor included 0.5 hides of Ely thegnland and 0.75 hides of Ely soke (para. 26/48);[43] but they were not concerned with Radulf's other hide, the soke of which belonged to the abbot of Ramsey. It was of no significance to them that Hardwin's men held a total of 34.625 hides in Cambridgeshire – and of even less significance that Hardwin's men held a total of 18.5 hides in Hertfordshire (para. 13), on none of which (as far as the evidence goes) did Saint Audrey think she had a claim. An Ely scribe might decide (as scribe T1 and others did later) to copy these paragraphs if they already existed; that is likely enough. But no Ely scribe would have had either the motive or the opportunity for bringing them into existence in the first place.

It has often been remarked that these summaries are similar to some that occur in a manuscript in Exeter – the same manuscript which largely consists of the surviving portion of the C text (Exeter Cathedral Library 3500).[44] In fact, to say that they are similar is to misrepresent the case: it would be more accurate to say that they are identical.

The summaries in question are contained in a two-leaf booklet (fos. 527–8) which got itself connected with the surviving C booklets. They were written out very neatly (527v–8r) by the scribe whom I call mu (above, pp. 41–2); but since this seems sure to be a fair copy, they may originally have been compiled by one of his colleagues, rather than by mu himself. What they give is a condensed description of the abbot of Glastonbury's lands in four counties: Wiltshire, Dorset, Devon and Somerset.[45] The program which generated these summaries is represented in Table 32.[46] Except that no distinction was made between

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[42] A note following para. 11 explains that the data being counted here (paras. 10–11) have already been counted in the paragraphs giving the grand totals (paras. 2–3): *Hęc t'ra predicta de Thainl' et soca est scripta*

*et appreciata in breue abb'is de eli* (T-112r). Similar notes follow paras. 14, 15 and 16. (There is no mystery about the expression 'the abbot of Ely's brief': it refers back to paras. 1–3.) An itemized list of the holdings in question, derived by an Ely scribe from xEl-Ca, can be found in one of the segments appended to xEl (T-146v–7v); the 'others' mentioned in paras. 2–3 can be identified from this. Erchenger the baker, for instance, is listed as the tenant of 1 hide of thegnland and 0.55 hides of soke.

[43] This is the paragraph quoted already (above, p. 98), as it appears in B / V and xEl / T.

[44] ((Galbraith, for example, got as far as saying that they are 'precisely similar' (1961, p. 116); but he only said it in a footnote, and then said nothing more. As far as I am aware, the first person to realize that they are practically identical, and that this fact cries out for an explanation, was Roffe (2000, pp. 181–2).))

[45] A poor copy of the fourth paragraph was inserted by a later (perhaps twelfth-century) hand in a blank space elsewhere (173r).

[46] The summary covering the lands of Saint Petroc in Cornwall (528v) is by a different hand, but follows the same program, minimally adapted to suit the case.

```
                    for each baron
                        for each county
                            summing over all manors held in domain
                                how many manors?
                                how many hides?
                                how many ploughs on the domain?
                                how many villains, bordars, slaves, others?
                                  and how many ploughs do they have?
                                how much is this land worth?
                            summing over all manors held by his knights
                                how many manors?
                                how many hides?
                                how many ploughs on the domain?
                                how many villains, bordars, slaves, others?
                                  and how many ploughs do they have?
                                how much is this land worth?
                            summing over all manors held by his thegns
                                how many manors?
                                how many hides?
                                how many ploughs on the domain?
                                how many villains, bordars, slaves, others?
                                  and how many ploughs do they have?
                                how much is this land worth?
                            summing over all manors
                                how many ploughs is this land sufficient for?
                                how much has it gained in value since it came
                                  into his hands?
                        next county
                    next baron
```

Table 32. Program for generating the summaries in Exeter Cathedral 3500, fos. 527v–8r.

knights and thegns, the summaries included with xEl were generated by exactly the same program.

If they have not had cause to think about such questions before, readers may think that one summary is bound to look very much like another. The answer to that (for those readers willing to admit that the thought did cross their minds) can also be found in the Exeter manuscript. A different scribe (not one whom I recognize) copied out another batch of summaries (530v–1r),[47] and the program for generating these is represented in Table 33 Though the loops are the same, the questionnaire is quite differently structured, as well as being much shorter.[48] Only three of the questions being asked here (the ones marked with an asterisk) are exactly equivalent to questions being answered in the summaries written by scribe mu.

In the wording too, we find some striking agreements between the summaries surviving in Exeter and the summaries surviving in this Ely text. Up to a point, of course, the word-

ing is determined by the content: there is only one way of saying 'ten hides', 'twenty ploughs', 'fifty villains'. As far as the xEl summaries are concerned, it is to be assumed that they had already been copied at least once before being copied into T, and we cannot be sure how accurately the surviving copy represents the original. Nevertheless, there are two specific agreements to be noted.

(1) In every county where the survey was carried out, one of the questions which had to be answered was this: how many ploughs would there be on this manor if the land were being fully exploited? It is not clear exactly how the question was worded, nor whether the wording was the same with respect to every county; the answers are variably phrased. In B-Ca, the formula used is *x car' ibi e' t'ra* or some permutation of that (*x car' e' ibi t'ra, t'ra e' ibi x car'*). The DB scribe, in the first booklet he wrote, DB-YoLi, experimented with several formulas – *quas poss' arare x car', et x car' poss' ibi esse, ubi x car' poss' esse* – before deciding that *T'ra e' ad x car'* was clear enough; but then he started wondering whether to omit the *e'* or the *ad* or both. The Exeter summaries use a distinctive formula:

*Hęc terra sufficit x car'* (four instances),

which (unless I have missed something) occurs nowhere else except in the xEl summaries, some of which (but not all of which) use the identical formula:

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[47] The second page (531r) is reproduced by Darlington (1955, opp. p. 217).

[48] The man who compiled these summaries was aiming for cumulative totals. In the case of Rotbert son of Girold, for example, he starts by giving the numbers already arrived at for Wi + Do; then he gives the numbers for So; and then he sums each pair of figures to get the numbers for Wi + Do + So (530v). Carrying on like this, he would eventually have arrived at numbers for the whole of England. To judge from the surviving copy, however, these were just desultory experiments; it is hard to believe that the compiler had enough momentum behind him to get very far with this plan.

```
for each baron
    for each county
        summing over all manors
            how many manors?
            how many hides?
            how many ploughs is this land sufficient for? *
            how much is it worth?
        summing over all manors held in domain
            how many hides? *
            how much is this land worth? *
    next county
next baron
```

Table 33. Program for generating the summaries in Exeter Cathedral 3500, fo. 531r.

*Hęc terra sufficit x car'* (seven instances).

(The other paragraphs replace this with *T'ra x car'* or *T'ra ad x car'*.) In both cases, moreover, the total reported is the total for all manors, regardless of whether they are held in domain or not.

(2) The Exeter summaries do not report the total value. We can work this out for ourselves, if we wish, by adding together the values reported for each category of manors (domain, knights, thegns); but the answer is not written down for us. Instead the compiler recorded a different number: not the value, but the change in value; not (as perhaps we might expect) since the time of king Edward, but since the time when the current owner got possession. If the value has increased, he writes that 'this land has improved by ten pounds in the hands of abbot Turstin':

*Hęc t'ra emendata e' in manu turstini abbatis x lib'.*

If the value has decreased, he writes that the land 'has worsened by ten shillings',

*e' peiorata de x sol'.*

If the value is unchanged, he writes nothing. Presumably what he did was add up all the *Modo* values, add up all the *Quando recep'* values, and then subtract the second sum from the first one. But only the result of this subtraction got recorded. The idea that one ought to be tracking the change in value seems to me a very sophisticated notion, and I do not know that we find any trace of it elsewhere – except in the summaries from Ely. At precisely the same point in the program, precisely the same calculation has been performed, and precisely the same formula is used for reporting the result. This land 'has improved by ten pounds in the hands of abbot Simeon':

*emendata de x lib' in manu Symeonis abb'is.*

The holdings covered by the xEl summaries had all gone up in value, and therefore we find no instance of the *peiorata* formula. Apart from that, I cannot imagine any better proof that these summaries – the ones in Exeter, the ones in xEl – were all compiled at the same time, in the same place, very

probably by the same man. And that means that they must have been compiled, from some version of the B text, in the king's treasury at Winchester.

How far this plan was followed through with, we are not in a position to decide. The summaries written by scribe mu look as if they may have been recopied for some special reason, perhaps in response to a demand for information on this particular topic. The summaries included with xEl are just the ones which caught the attention of a visiting Ely scribe. Even so, these accidental survivors span eleven counties (including Saint Petroc's lands in Cornwall), and it would be a strange fluke if these were the only counties for which summaries had been compiled. On the other hand, it would hardly seem sensible to draw up briefs of this kind except for baronies of a certain size. It is not impossible – I do not think it should be put more strongly than this – that the Treasury officials drew up a summary for every important baron, for every county where he was a holder of land. That it could have been done is clear: how far it was done we are never going to know.

But that, in a sense, does not matter. Regardless of how far the program was carried out, the program itself is significant. It tells us what the Treasury thought about the survey. These, in the Treasury's view, were the most important facts, and this was the most convenient way for those facts to be arranged. For anyone who hopes to understand what purposes the survey was intended to achieve, this is crucial evidence. Thanks to Ellis, it has all been available in print for nearly two hundred years: perhaps someone should finally make use of it.[49]

# 6

By way of conclusion, I give a short account of the history of xEl, as far as I think I can make sense of it. Much of what follows is guesswork, and I do not pretend otherwise; but it seems to me that the account given here is more coherent, and more consistent with the facts, than any alternative

[49] ((For the reader's convenience, I have reprinted both stretches of text in Appendix II.))

account that I can think of. I have simplified it in some respects; no doubt I have made it too simple.

The story begins in Winchester, with the arrival at the Treasury of a scribe from Ely (perhaps one of the monks, perhaps an employee of the abbot's). It is not clear when we want to think this happened; but probably it happened in abbot Simeon's time, i.e. no later than 1093. The man from Ely brings with him a letter (perhaps from the king, perhaps from someone else) ordering the Treasury officials to give him access to the commissioners' reports (the B text) for those counties in which Ely has an interest, and to let him make whatever excerpts he likes.[50] The Treasury officials comply. Working quickly but not in a rush, the Ely scribe combs through the commissioners' reports, finding and copying every paragraph which makes any mention of Ely. (For some of the time at least, he has a companion working alongside him.) While he is in Winchester, he comes across a collection of summaries compiled by a Treasury scribe, and copies some of those as well. He then returns home with a batch of booklets containing all the excerpts that he has made, perhaps intending to copy them out again more neatly, when he has the time. But that intention, if it existed, comes to nothing.

For the next fifty years, these booklets sit on a shelf in Ely. They are in the monks' custody by now; they are not forgotten. From time to time, for one reason or another, somebody looks through them, extracting some information which seems of interest to him; and a number of these derivative texts attach themselves to the bottom of the stack of booklets.

At some stage, a portion of the B text becomes available in Ely. If anyone wishes to visualize it as a collection of rolls, I have no objection at all. Simply to fix ideas, let us think of it rather as a bound volume, B-Ca..Ht.., of which we can stipulate three properties: it begins with Ca; it contains one further county of interest to Ely, namely Ht; it does not contain any of the other four, Ex, Nk, Sk or Hu. We can also be sure that it arrived in Ely before 1140; so the likelihood is that it arrived at the same time as bishop Nigel, who had earned his promotion to the episcopate by serving as the king's treasurer. (Why only one B volume arrived is probably not worth asking: there are too many possible answers. Perhaps the other volumes no longer existed; perhaps the Treasury would not release them; perhaps Nigel only wanted to borrow this volume, the one which covered Ely itself.) Within the next few years, somebody copies some extracts from this book. Perhaps he copies the title, *HIC SUBSCRIBITUR INQUISITIO TERRARUM* … ; we

do not insist on that.[51] What we are definitely guessing that he does is this: he copies the lists of jurors' names for every hundred in which Ely has an interest. That may look like an odd thing to do, fifty years after the event, when hardly any of the jurors can still have been alive; but it may have seemed less odd at the time, when the new bishop was trying to assert his authority, both against the monks and against the church's tenants. Whatever the motive for making them may have been, these extracts from B attach themselves to the top of the stack of xEl booklets.

Eventually, in 1139–40, scribe T1 makes a copy of the whole stack of booklets, turning the entire contents into one consecutive text. Some time later, the booklets cease to exist, but this copy survives; and two other copies made from this one also survive. That is the end of the story, so far as xEl is concerned.

Meanwhile the bound volume of B (or the collection of 'hundred rolls', whatever it may have been) is not returned to the Treasury: it remains in Ely, and finally scribe V1 decides to make a copy of it. Perhaps he copies the whole text, covering several counties; perhaps he copies only part of it; we do not know and never will. The original then disappears, and so does some part of the copy that has been made of it. All that survives is a fragment of this copy, broken off at a point about three-quarters of the way through the first county. And that is the end of the story, so far as the B text is concerned.

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[50] Probably the letter came from someone else, with authority to issue such orders: if the letter had come from the king, we would expect it to have been preserved. One of the writs which does survive (Bates 1998, no. 127, to be punctuated in the manner suggested by Round (1895, p. 133)) is important in several respects – as proof, for instance, that abbot Simeon was displeased with the treatment accorded to him by the commissioners – but cannot be connected specifically with the compilation of xEl.

[51] Not knowing where this passage came from, we cannot think of relying on it. Does that matter? If we want to know what questions were being asked, we can work things out for ourselves, from the evidence of B-Ca; it would be just laziness to quote this passage instead.